Article:

Koppensteiner, M.F., Matheson, J. and Plugor, R. (Accepted: 2022) The impact of improving access to support services for victims of domestic violence on demand for services and victim outcomes. American Economic Journal: Economic Policy. ISSN 1945-7731 (In Press)

The impact of improving access to support services for victims of domestic violence on demand for services and victim outcomes*

Martin Foureaux Koppensteiner† Jesse Matheson‡ Réka Plugor§

October 13, 2022

Abstract

We conducted a randomised controlled trial of an intervention designed to assist victims of domestic violence in accessing non-police support services. The intervention led to a 22% decrease in the fraction of victims providing a witness statement to police. Witness statements are an important piece of evidence and a key input in the prosecution of perpetrators. Despite this, we do not find a significant change in perpetrator arrests and convictions or in reported future violence. Survey responses provide evidence of an increase in non-police service use, a reduction in future victimisation risk, but also a potential decrease in short-run well-being.

Keywords: Support services, domestic violence, RCT, allocative efficiency

JEL Codes: I18, J12, H75

---

*We are grateful for comments provided by Anna Aizer, Sofia Amaral, Dan Anderberg, Orazio Attanasio, Herb Emery, Gianni De Fraja, Esther Duflo, Mark Hoekstra, Pamela Jervis, Magne Mogstad, Krzysztof Karbownik, Melanie Khamis, Tom Kirchmaier, Bettina Siflinger, Chris Wallace, Tanya Wilson, and seminar participants at the Asian Meeting of the Econometric Society, the Bristol Workshop on Economic Policy Interventions and Behaviour, the Essen Health Conference, the IZA Workshop on Family and Gender Economics, the Royal Economic Society Meeting, Royal Holloway UL, SITE Stockholm, and the Warsaw $(\mathrm{Ce})^2$ workshop. We thank Chloe Rawson for providing exceptional research assistance. We thank the Leicestershire Police, the Office of the Leicestershire Police and Crime Commissioner, and Leicester City Council for enabling the evaluation as a randomised controlled trial and for data access. We gratefully acknowledge funding from the UK Ministry of Justice PCC Fund. AEA RCT Registry No. AEARCTR-0000537. An earlier version of this paper was circulated under the title 'Public Services Access and Domestic Violence: Lessons from a Randomized Controlled Trial'.

†School of Economics, University of Surrey, Guildford, GU2 7XH, UK

‡Department of Economics, University of Sheffield, Sheffield, S10 2TN, UK, j.matheson@sheffield.ac.uk, +44 114 222 3310

§School of Business, University of Leicester, Leicester, LE1 7RH, UK

1 Introduction

Domestic violence (DV)[1] is a problem of first-order importance across the world, with recent estimates suggesting that almost one-third of women have been subjected to violence by their intimate partner at one point in their lives (World Health Organization, 2021). In 2018, there were 1.2 million reports of domestic incidents to police in England and Wales, leading to approximately one-third of all arrests made by police forces (Home Office, 2019; Office for National Statistics [ONS], 2019). These numbers do not include the many more cases never reported to the police (Gracia, 2004). Of those cases reported to police, less than six per cent resulted in the conviction of a perpetrator. While the scale of the problem of DV is vast, progress in identifying interventions to reduce the incidence of DV and improve the well-being of victims has been modest.

The large number of DV cases include many repeat incidents involving the same households.[2] This situation comes amid the availability of several public and private non-profit organisations that help victims of DV address long-term abuse by providing services and opportunities to change personal circumstances.[3] A potential problem that has received a great deal of attention by practitioners is that victims are unaware of these services or find it difficult to access them.[4]

In this paper, we provide randomised evidence on whether improving victim access to existing services will lead to better DV outcomes. We consider several margins along which

[1] 'Domestic violence', as used here, refers to both intimate partner violence and violence between family members.

[2] The 1,015 households in our sample make up 3,342 police reported cases over a year. This is about 20% of the total of 17,000 DV cases reported to Leicestershire police in 2013.

[3] These services include the provision of temporary housing and women's shelters, help with permanent housing, designing of and helping to implement escape plans to leave a violent partner, legal aid, and other forms of support to leave a perpetrator.

[4] Victims' lack of information was highlighted in a report on the policing of DV by the UK police watchdog Her Majesty's Inspectorate of Constabulary and Fire & Rescue Services(HMIC, 2014) as well as Fugate et al. (2005), who show that information barriers are a significant deterrent for victims of DV in the United States.

we may expect to see better outcomes, including repeat violence, victim well-being, and changes in the use of police and non-police services. We worked with a large UK police force to implement a randomised controlled trial (RCT) of an intervention specifically designed to improve access to non-police support services. The trial focused on households that have experienced multiple reports of DV over a period of 365 days. The intervention provided victims of police-reported DV with a caseworker who offered information about and assistance in accessing support services. The trial lasted for six months and randomisation took place at the level of individual victims. The final sample of over one thousand households constitutes one of the largest RCTs on DV to date.

Our analysis is based on a unique dataset that we constructed by linking information from several sources. The dataset includes information from local police administrative records on reported DV outcomes over a two year period, information from the UK Police National Database on perpetrator convictions and sentencing, and information from a victim follow-up survey capturing victim-centred outcomes such as the future risk of violence and other measures of well-being. These extraordinarily rich data allow us to follow the lifecycle of every case in our sample, from the time a case is opened to the time a perpetrator is sentenced in court.

Three significant findings emerge from this study. First, treatment group victims were significantly less likely to provide police with a formal account of the incident, known as a witness statement (*statement* henceforth).[5] This is important, as victim statements are a critical, and often the only, piece of evidence in building a case against a perpetrator. We find that the intervention led to an intention-to-treat effect of a 6.5 percentage point decrease (21.7% relative to the control group mean) in statement provision. Using information on the precise timing of when statements are provided to police, we show that while there is no

[5] A witness statement is written or recorded oral account of what happened to a victim, providing police with evidence on a potential crime which may be used later in court. Providing a statement to police would be analogous to what is commonly called ‘pressing charges’ in the North American context.

difference in the statement provision on the day police visit the household in response to the reported violence (before treatment status is assigned), the number of statements made after the treated group received the intervention differs significantly. Using data collected through a victim survey one month following the intervention, we also find that the treatment group is more likely than the control group to report using non-police support services. Taken together, these results are consistent with the victims in this study using non-police support services and police services as substitutes.

Second, despite the decrease in statement provision, we do not find an effect of the intervention on criminal sanctions against a perpetrator (specifically, arrest by the police, charges by the Crown Prosecution Service, and sentencing by the courts). This is surprising, as the correlation between the provision of a statement and criminal sanctions against a perpetrator is positive and strong. This suggests that for victims who make a statement in control, but not in treatment, the effect of their statement on criminal sanctions is low relative to other victims. One plausible explanation for this is that these victims are more likely to retract their statement, making their statement inadmissible as police evidence. Relative to the control group, treatment group statements are 10.3 percentage points, or 84%, less likely to be retracted. We interpret this result as the intervention increasing the efficiency of police service utilisation by removing ineffective statements from the service load of police officers.

Third, different from previous secondary responder programmes,[6] of which some reported an increase in repeat victimisation, we do not find a significant effect on repeat police-reported household violence over a two-year period. Specifically, treatment group households are as likely as control group households to experience a repeat police callout (77.8% and 74.9% respectively), and have approximately the same number of repeats (3.0 and 2.7 on

[6]In secondary responder programmes, teams often composed of police officers and social service workers or victim advocates conduct follow-up visits with victims of domestic abuse following a police call-out of DV. Peterson et al., (2022) provide a summary of the previous literature on secondary responder programmes.

average). Furthermore, duration analysis suggests that the timing of repeat reporting does not significantly differ between the two groups. Across several measures that reflect the severity of future cases, we find suggestive evidence that reported treatment group cases are less severe. For example, we find that reported scores from a standardized tool for assessing violence escalation risk by responding officers are 9.5% lower for the treatment group cases than for the control group cases. While this difference is only marginally statistically significant, taken together with the other measures of incident severity, this provides evidence consistent with the intervention leading to an increase in the willingness of victims to report less severe future incidents to police.

This interpretation is supported by estimates using supplementary data specifically collected through a victim survey one month following the intervention to study outcomes not found in administrative records. Using the victim survey data, we find an increase in the willingness to report any future incidents to the police. We also find that the treatment group has an overall lower risk of repeat victimisation. In particular, we find that individuals in the treatment group are more likely to report no longer being in contact with the perpetrator. Finally, we find that the treatment group is more likely than the control group to report using non-police support services. Despite these positive findings of the intervention on the safety of the victim, we find that reported measures of stress for treatment group individuals are higher than for control group victims one-month after the initial incident, possibly indicating the increase in stress associated with changes in the personal circumstances of the victim engaging with DV services.

The theoretical basis for the intervention design is based on a household bargaining model (Aizer, 2010; Anderberg and Rainer, 2013; Anderberg, Rainer, Wadsworth and Wilson, 2016; Bobonis, Gonzalez-Brenes, and Catro, 2013; Farmer and Tiefenthaler, 1996). If support services improve the outside options available to victims of household violence, we expect that making these support services easier to access will lead to a decrease in violence. As it is not possible to directly observe violence in the household, we must attempt to infer changes based on reporting behaviour and our survey response. Our results are consistent with the intervention leading to a rise in willingness to report future incidents, with the number of reported incidents increasing and the severity of reported incidents decreasing.

This paper contributes to two strands of literature. The first is literature studying the role of public policy to improve DV outcomes. In particular, several studies use experimental designs to analyse *secondary responder* programmes, in which police officers or officer/social worker teams follow up on households after an initial report of violence (Casey, et al, 2007; Davis and Taylor, 1997; Davis, Weisburd and Hamilton, 2010; Hovell, Seid and Liles, 2006; Stover, Berkman, Desai and Marans, 2010; Stover, Poole and Marans, 2009). These studies unanimously find that secondary responder programmes, at best, do not lead to a change in household violence and may even increase household violence. The intervention studied here differs from previous research in several significant ways. The intervention was designed with the primary goal of making support services more accessible to victims. Steps are taken to ensure this is done without involving the perpetrator. The intervention caseworkers were domestic abuse specialists with extensive local knowledge and experience with accessing existing services, rather than police officers as in previous secondary responder programmes. Because the caseworkers were embedded within the police, they still benefited from access to police intelligence. The study also differs from previous work in ways that improve our ability to infer programme effects. First, the RCT design here does not allow for any override of random assignment to treatment. Previous RCTs (Davis and Taylor, 1997; Davis, Weisburd and Hamilton, 2010) involved a small number of treatment assignment overrides by the police, which may have led to estimation biases. Second, the RCT studied here has a sample size more than double that of previous RCTs. This allows us to estimate significantly smaller effects that previous studies may have missed. Third, the dataset built for our analysis is based primarily on police administrative records covering a two-year period. These records provide a standardised measure of the severity of each incident. This standardisation allows us to answer the question of whether the intervention reduced repeat incidents or changed reporting patterns by assessing the severity of the subsequent incidents attended by police.

This paper both contributes to and is informed by a second strand of literature studying barriers to public service uptake and interventions to overcome those barriers. The first set of papers focuses on information interventions in a variety of experimental settings. For example, studies have examined how simplifying information on public school performance leads parents to select higher-performing schools for their children (Hastings and Weinstein, 2008). Research has also addressed how the provision of information on the cost and benefits of education changes students' intention to stay in non-compulsory education (McGuigan, McNally, and Wyness, 2016) as well as increasing enrolment in post-secondary schooling for unemployment insurance recipients (Barr and Turner, 2018). In addition, researchers have examined how personalised prescription drug plan information makes Medicare users more likely to switch to lower-cost plans (Kling, Mullainathan, Shafir, Vermeulen, and Wrobel, 2012).

Another set of papers focuses on interventions that assist persons in accessing services. For example, Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012) conclude that assistance in filling out complex college aid applications leads to an increase in college enrolment. Research has also shown that removing assistance in completing disability applications from closures of field offices led to a persistent decline in the number of disability recipients (Deshpande and Li, 2019). In practice, information and assistance often significantly interact. Finkelstein and Notowigdigdo (2019) study the effects of providing information and assistance aimed at increasing the take-up of SNAP benefits in Pennsylvania. They find that information alone increased enrolment in SNAP by 5 percentage points, but when combined with assistance, enrolment increased an additional 7 percentage points.

These studies demonstrate that relatively simple information and assistance interventions can help overcome bureaucratic barriers or costs to obtaining and processing information. These interventions reduce distortions in choice compared to what is selected without administrative and financial barriers. Our study is similar in spirit, considering a relatively simple and inexpensive change to the way that victims of DV receive assistance following a police-reported incident. If victims of DV find it difficult to access services or determine which services are best suited for their needs, then they may rely on simple heuristics such as utilising police services with which they already interact. Unlike previous studies, we consider service users who choose among different, non-exclusive services. Potential service users can and do choose more than one service. Services are not explicitly priced; therefore, users do not internalise the cost of service provision and may allocate themselves in such a way that service costs outweigh the private benefits. This is a general problem with any publicly available service.[7] If the cost of providing police services is high relative to non-police services, then the intervention is likely to improve allocative efficiency. This is particularly important for services related to DV because of their frequency and relevance to policing. In the UK, DV and abuse account for approximately 11% of all crimes reported to police,[8] creating substantial service demands on police forces in the country.

There are limitations to the interpretation of our results. Specifically, we do not generalise beyond the selection criteria for our subject pool, namely focusing on households with previous police-reported domestic incidents. This study does not attempt to draw any conclusions about the use or effectiveness of public support services for households going through their first police-reported DV incident, or households with unreported DV. Moreover, the households in our subject pool received treatment at most once. We cannot conclude whether repeated access to the programme would have different results. Finally, as with other studies that use police administrative records, we only observe incidents reported to police. This

[7]This problem may, for example, show up in the case of school selection or attendance, as in Hastings and Weinstein (2008) or Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012).

[8]This number is based on official statistics provided by ONS (2018a, 2018b).

will limit what we can say about the unobserved violence experienced in our households. In Section 5 of the paper, we provide a discussion of the interpretation of our results with this in mind.

The paper is structured as follows. Section 2 provides the contextual background information for the experiment. Section 3 provides details of the RCT design and implementation, followed by data sources and collection. The main results of the paper are presented in Section 4. A discussion and interpretation of results is offered in Section 5, and an analysis of the intervention cost appears in Section 6. Section 7 summarises the findings and places them in a wider context.

2 Background

Non-police services available to victims of DV

In the UK, DV support services are available through a number of publicly funded and voluntary service providers. In the police force area we study (Leicestershire, UK)[9], 24 different agencies provided various DV support activities at the time of the intervention. In Appendix A, we provide detailed information about the available services, including a table summarising all DV service providers, a list of the categories of services most accessed by the treatment group in this study, and the service information pamphlet that police provide to all victims following a domestic incident.

Several barriers make it costly for victims to access these services.[10] These barriers can arise from four non-exclusive sources. First, victims may lack information about the existence and availability of these services or the process to access them. Second, barriers may arise

[9]This police force area covers three local councils, roughly comparable to US counties, Leicester City, Leicestershire and Rutland. We henceforth use *Leicestershire* to denote the common administrative area covered by the Leicestershire police force

[10]In Appendix B.1 we provide a stylised conceptual framework to guide our thinking about the relationship between access barriers and the choice between various DV services.

from the complexity of choice over the often large set of services, similar to that explored in Hastings and Weinstein (2008) and Kling et al. (2012). Third, barriers may originate at the individual level from psychological or language barriers. Fourth, to ensure the safety of users or restrict the use of scarce resources, service providers often establish burdensome formal procedures, such as the requirement for a gatekeeper referral to access some houses of refuge.

While we do not explicitly distinguish between sources of barriers, they are widely recognised as an impediment to service uptake.[11] The intervention we study is specifically designed to help victims of DV overcome these barriers by providing information on existing services, signposting victims to the appropriate service, helping them overcome psychological and language barriers and providing referrals to these services.

Police services available to victims of DV

We refer to police attending a DV incident in response to an emergency call made by a victim or a third party as the *initial callout*. When police officers attend an initial callout, they have two tasks. The first is to defuse the immediate and potentially volatile situation and ensure the safety of all individuals involved.[12] The second task is to collect evidence at the initial callout to determine whether to initiate further investigations for pursuing criminal sanctions against the perpetrator. Evidence can be direct, such as police observing and recording a physical assault through body-worn cameras. More often, however, evidence is

[11] *Her Majesty's Inspectorate of Constabulary* (HMIC, 2014) reports anecdotal evidence based on subject interviews that victims of DV felt that they did not know where to turn for help after an initial police callout. These barriers to services are not unique to the UK context. For example, in the United States and Canada, Jaffe et al. (2002) show that 'women reported feeling let down and confused by the [community and social services support] process'. The authors find that many women removed their application for services out of frustration with the number of barriers. In interviews with DV victims in Chicago, Fugate et al. (2005) find that perceived barriers to access, particularly a lack of information, are a significant explanation for whether victims contact social and counselling services. However, these barriers are not important for explaining why victims contact police services.

[12] Police have the power to arrest and temporarily detain a perpetrator for up to 24 hours solely for this purpose. This arrest may be made independent of the victim's preferences. After 24 hours, either formal charges must be laid or the perpetrator must be released.

indirect in the form of statements made by witnesses, including any victims. A statement is a recorded recollection of events by a witness that can be used as evidence in court.

We use *police services* to refer to the further investigative work undertaken to pursue criminal sanctions against a perpetrator. In contrast to the two tasks outlined above, which are performed at every initial callout, further investigative police services are only performed if there is reason to believe that there will be sufficient evidence for the *Crown Prosecution Service* (CPS) to pursue formal charges against a perpetrator. The CPS decides whether to charge the perpetrator to pursue criminal sanctions on the strength of the available evidence.[13]

Figure 1 about here

The decision to provide a statement is a mechanism through which the victim can influence the progression of the case towards criminal sanctions against the perpetrator. In the majority of DV cases, the victim is the primary witness, and the victim's statement is the major piece of available evidence. In the absence of other witnesses, a victim statement is used both for charging a perpetrator and as evidence in court proceedings, giving it a key role in prosecuting perpetrators of DV. A victim can provide a statement at the initial callout (in our data, 50.4% of victims who provide a statement do so at the initial callout), or a victim can contact the police and provide a statement any time after the initial callout.

Once a statement is provided, the victim may decide to retract the statement at any time until the CPS decides to pursue charges.[14] In our data, 17.0% of all statements are retracted. If this retraction occurs, the statement cannot be used as evidence in the case against the

[13]Evidence of this screening process is found in the data (Figure 1). In cases where charges are laid, 60.8% result in sentencing by the courts (including prison time (24.4%), fines (13.7%), restraining orders (39.2%), and mandatory rehabilitation programmes (17.3%)). There is no significant difference for cases in which a statement is made (61.9% versus 50.0%). This finding is consistent with the role of the CPS in filtering cases that proceed to the courts based on the strength of the evidence.

[14]Once the CPS has decided to pursue charges, victims can add to, but may not be able to retract, their statement (CPS, 2021).

perpetrator, and often charges against the perpetrator will be dropped.[15] Many reasons motivate the retraction of statements, including a fear of repercussions by the perpetrator or other family members, lack of information on the criminal process, fear about immigration status, and remorse expressed by the perpetrator (CPS, 2021; McGuire, Evans and Kane, 2021; Robinson and Cook, 2006). Aizer and Dal Bó (2009) provide evidence from no-drop policies in the United States that statement retraction in DV is consistent with a model of time-inconsistent preferences, in which a victim's value from prosecuting the perpetrator is strongest shortly after the initial callout but declines over time. A belief in time inconsistent preferences also appears to underlie the policy studied by Ford (1983), in which US prosecutors impose a three-day cooling-off period to allow the victim to 'assess her options'.

The correlation between the provision of a victim statement and perpetrator charges and arrests is strong (see Figure 1). For our data, in the 743 cases for which no statement was provided, the perpetrator was subsequently arrested in 9.7% of cases and charged in 3.5%. In the 272 cases where a statement was provided, the perpetrator was arrested in 68.0% and charged in 39.0%. These data, however, do not reveal anything about the causal effect of statements on arrests and charges. Victims may choose to make a statement based on their subjective expectation of the probability of an arrest. The correlation does provide evidence of the significance of statement provision in pursuing punitive action against a perpetrator.

3 Experimental design and data

We conducted an RCT in the English county of Leicestershire jointly with the Leicestershire Police Force and the three governing authorities which make up Leicestershire.[16] Leicestershire (see Figure 2 for map) covers a population of approximately one million people,

[15] The charity *Rights of Women* advises victims regarding the provision of a victim statement in DV cases that 'Without a witness statement from you, it is unlikely that the police will continue'. (Rights of Women, 2013).

[16] Leicester City Council, Leicestershire County Council and Rutland County Council.

and the Leicestershire police force is one of 43 police forces in England. One-third of the population in Leicestershire is concentrated in the city of Leicester, with the remaining population distributed across approximately 300 towns and villages. The experiment ran between November 2014 and April 2015.[17].

Figure 2 about here

3.1 Allocation of cases into the subject pool

We worked with the Leicestershire Police IT services team to design an automated computer application for selecting the subject pool and assigning treatment.

After responding to a domestic incident, officers record a domestic incident report in the Leicestershire Police database. Our automated application performed a daily scheduled search for all newly recorded incidents. The recovered domestic incident cases must have met several conditions for inclusion in the subject pool: (1) the report was filed as a *domestic incident*; (2) in the previous 365 days, the victim had shown up in at least three and fewer than seven DV reports (including the current one);[18] (3) the victim was not previously in the subject pool (in either the treatment or control group); and (4) responding officers did not recommend the victim for a pre-existing intervention known as a *Multi Agency Risk Assessment Conference* (MARAC).[19] All cases that met these criteria were assigned to the subject pool. The application automatically allocated subject pool cases to treatment or control groups, each with a 50% probability. During the trial period, more than 50 reported

[17]The experiment and the data collection received approval from an Internal Review Board at the University of Leicester. We provide details on the application in Appendix section C.

[18]The initial interest of this intervention was to assist victims of repeated DV. The minimum of three offences was based on the predicted capacity constraints of the trial. If more than seven DV incidents occurred in the households, the case was potentially referred to a separate pre-existing intervention as a standard procedure.

[19]MARACs are used UK-wide. During a MARAC, information on the highest-risk domestic abuse cases is shared between representatives of police services, health care, child protection, housing practitioners, probation and other DV specialists from the statutory and voluntary sectors. These stakeholders discuss options for a coordinated action plan to increase victim safety.

domestic incidents were recorded daily with seven, on average, qualifying for the subject pool. To examine statements and conduct the survey, the person labelled *victim* in each case report was assigned as the subject.

The final sample consists of 1,017 cases, with each case referring to a unique victim. Of these, two cases were dropped due to restrictions on access to police data.[20] A few cases did not have information on all control variables. For the regression analysis, these missing values are given a value of 0, and a variable-specific dummy will be used to indicate the missing values.[21] The final dataset for our analysis consists of 1,015 unique cases. Of these, 510 cases are in the treatment group and 505 are in the control group.

3.2 Control

All cases in the subject pool received standard police procedure as described in Section 2. Upon attending the initial callout, responding officers left a pamphlet with victims that lists, describes and provides contact information for some of the available DV services in Leicestershire (see Appendix A). Victims could contact the services on the pamphlet at any time. Victims were also invited to provide a statement to police any time during or after the initial callout.

3.3 Treatment

A treatment group case was assigned to a caseworker the morning following the recording of the case in the police database. Cases were allocated non-randomly among the caseworkers according to workload and availability. Three dedicated caseworkers were employed for the trial. The caseworkers were female and between the ages of 25 and 35. Caseworkers all had previous training and experience as domestic abuse support workers. Specifically, all had

[20] This redaction would happen in a situation where individuals in the case are under investigation for a serious offence such as sexual assault involving a minor.

[21] Reported results are robust to the exclusion of missing variables from the analysis.

previous experience working with DV support services in Leicestershire and had specialised knowledge of the various local services available and how to access them. The caseworkers also received training specific to the service provided through the intervention in this study.[22] Caseworkers were provided with desk space and IT support in a large Leicestershire Police station.

The caseworker attempted to contact subjects via telephone within 24 hours[23] of the initial police report. Once contact was made, the caseworker described the public support services locally available to the subject. If the subject desired to access a specific support service, the caseworker assisted in initiating access. This assistance included organising initial contact with the relevant support service, helping complete any paperwork and providing a referral when necessary. All contacted subjects were offered a face-to-face meeting with the caseworker to go through the options available. If the subject expressed an interest to leave the perpetrator, the caseworker also assisted in preparing an escape plan. The intervention ended when either the subject declined to participate in the intervention or a relevant support service had taken up the case.

Although the specific content of each interaction varied by case, important features of the intervention were common to all cases. First, a caseworker attempted initial contact with subjects within a short period (24 hours) after the police report of the incident was filed. Second, caseworkers had access to all police information about victims and perpetrators, including historical police records. Third, subjects were informed of available non-police services, and if they wished to move forward, caseworkers assisted with accessing these services.

We define a subject as having *engaged* with the intervention if they were successfully

[22] One of this study's authors was present during these training sessions.

[23] While caseworkers were on duty and attempted to make contact on Saturdays, subjects of incidents occurring between Saturday evening and Monday morning were all contacted on Monday, thus extending the period of first contact to 36–48 hours in these cases.

contacted by a caseworker and they accepted some form of assistance, ranging from the provision of advice during a one-time phone conversation to face-to-face follow-up meetings. While an effort was made to deliver the intervention to all 510 subjects assigned to the treatment group, 249 (49%) of treatment group subjects did not engage.[24] Of these attempts, 107 victims were contacted by a caseworker by telephone but refused both phone-based assistance and a face-to-face meeting. For the remaining 142 subjects, caseworkers were unable to make contact given the available contact information.[25]

Among subjects whom the caseworkers were able to contact, the engagement rate was 65% (we explore characteristics of those who engage in the following Section 3.7 and in Appendix F.2). Considering that caseworkers 'cold-called' the subjects, this is a notable take-up rate and similar to the engagement rate for other assistance interventions such as those studied in Finkelstein and Notowidigdo (2019) and Bettinger et al. (2012)[26]. Of the 261 subjects who did engage, 128, or 49%, had at least one face-to-face meeting with the caseworker. Just under 35% of all home visits took place within 24 hours of the initial callout (the same day that caseworkers made first contact), with another 20% occurring within three days. In all, 33% of home visits occurred after three days but within a week, and the remaining 13% took place more than one week after the initial callout.[27]

[24]A maximum of five attempts made at different times of day across five days, were made to contact victims by phone.

[25]For the subjects' safety, the caller ID was not displayed, which may have led to some subjects not answering the call.

[26]Similar to our intervention, the intervention in Finkelstein and Notowidigdo (2019) assisted eligible individuals with the application process by telephone, achieving an engagement rate, conditional on calling, of 60%. Bettinger et al. (2012), who provide application assistance in person, achieve an engagement rate of almost 70%. This contrasts with much lower response rates in interventions providing information via a letter only. Bhargava and Manoli (2012) find an overall 25% response rate in an EITC benefits experiment, while Barr and Turner (2016) report a response rate between 2–3% to letters sent informing individuals who recently experience job loss, on opportunities from postsecondary programmes. While the type of intervention and context certainly are an important factor for the response rates, that assistance was offered directly from a real person in the former interventions probably also plays an important role.

[27]In Appendix B.3, we provide and test an alternative rationalisation of our main results based on the timing between the initial callout and the visit by the engagement worker, creating a cooling-off period, which decreases the provision of statements. We show that the data do not support this rationalisation.

3.4 Internal validity

Several design features of the trial safeguard the internal validity of the study. Most importantly, all assignments to the treatment and control groups were automated and randomised. Unlike previous RCTs of similar second-responder interventions (Davis and Taylor, 1997; Davis, Weisburd, and Hamilton, 2010), caseworkers or police officers could not override assignment to treatment.[28]

The timing of the treatment assignment occurs after the initial callout when the responding officer records the case in the police database. This ensures that the actions taken by police at the initial callout were not influenced by knowledge of the treatment assignment. Furthermore, this procedure provides a falsification test, which we exploit in Section 4.1, as statements made during the initial callout cannot be influenced by the treatment status.

Caseworkers only received information on cases in the treatment group. While caseworkers could have theoretically searched police reports for other reported DV cases on their own initiative, we are confident this did not happen. Every access to a report in the police information system is recorded and monitored, and unauthorised access to cases not in the treatment group by the caseworkers might have resulted in disciplinary action.

3.5 Data

This study is built around a unique and innovative data set that we constructed from three sources, briefly discussed below.

[28]Police officers did not have access to information on the treatment status of victims of DV. Furthermore, based on informal discussions with members of Leicestershire Police, most officers responding to DV calls were unaware of the intervention during the trial.

Leicestershire Police Database

We matched cases in the subject pool with Leicestershire Police administrative records, from a number of internal databases (detailed in Appendix D), using a unique crime reference number. The administrative records from these databases provide information on the initial incident (date, time, location, attending police officers, provision of a statement by the victim, and actions taken by police) and a wealth of information on the victim and perpetrator, including demographic characteristics, household information, and previous and subsequent police records.

For each reported case, responding officers assessed the risk of violence escalating using a tool, standardised across all UK police forces, known as domestic abuse, stalking and honour-based violence (DASH) assessment.[29] We collected the risk assessment score of the responding officer, taking values of 1 for the lowest risk and 3 for the highest risk. We also collected the raw DASH score reflecting the total number of risk criteria (out of a possible 20), which the responding officer recorded as affirmative. A higher DASH score means that the victim meets more of the criteria on which escalation risk is assessed. We interpret a higher DASH score as indicative of a more severe incident.

We collected information on reported incidents involving victims in the subject pool for two years following the intervention. Personal identifiers, including name, date of birth and address, were used to link information for victims and perpetrators across different cases over time.

The information was collected by three research assistants who did not have information on the treatment status of individual cases.[30] A fourth research assistant checked the

[29] The DASH assessment tool is based on a series of 20 yes/no questions that the responding officer asks victims of domestic abuse. The tool is used as guidance for referring cases to a MARAC meeting to manage the risk. We provide an example of the questions of the DASH assessment tool in supplementary Appendix I.

[30] IT and data protection training was provided by Leicestershire Police to the research assistants and the authors over a three-day workshop prior to data collection. Because of the sensitive nature of the data accessed in these databases, research assistants and the authors went through police vetting and criminal

recorded information for consistency and accuracy from a random draw of approximately 30% of the cases.

Police National Database

Outcomes of the criminal justice process are not contained in the administrative records of Leicestershire Police. This information is only available from the Police National Database (PND), designed to share intelligence across all police forces and criminal justice agencies throughout the UK. The PND holds over 3.5 billion searchable records with information about individuals who have been arrested, charged, and convicted. The nationwide coverage allows us to track individuals beyond the Leicestershire Police Force area and access information on all convictions of individuals.

The unique crime reference number given to each case allows us to link information from Leicestershire Police records to information from the PND. These linkages were cross-checked by the recorded date of the incident. We collected information on whether a perpetrator was arrested by police during or following a DV incident, whether the perpetrator was charged by the CPS, and whether a perpetrator was sentenced in court for the incident (along with details of the sentencing). Prosecution and court information was accessed more than 24 months after the randomised intervention took place, to allow for criminal justice proceedings to be completed.

Because access to the PND is highly restricted, even within the police force, the data were collected by a specially trained and licensed police officer for whom every access to the PND was authorised for the research project. This officer was blind to the treatment status of individual cases.

---

background checks. All research assistants were undergraduate students at the University of Leicester with a background in law or criminology.

Victim survey

Outcomes of interest relating to victim safety, well-being and non-police service use are not available from administrative sources. For this reason, we designed a victim survey to collect supplementary information.[31] However, there were important practical and ethical implications for the repeated collection of sensitive survey information from DV victims. For this reason, our data collection was limited to a single application of the survey one month after the intervention.

The victim survey was conducted by the Leicestershire Police Information Services Unit using researchers specifically trained in surveying victims of crime. Interviewers conducted the survey blind to the treatment status of the interviewee. Surveys were administered approximately one month following the initial callout and completed over the telephone using the safe number provided to police at the initial callout.[32] The survey was administered to a 25% random sample of the full subject pool.[33] From this sample, we received an 84% response rate, resulting in complete surveys for 105 treatment group subjects and 109 control group subjects.

In Appendix E.2, we provide a detailed analysis of selection into the survey sub-sample, and the potential for related biases. We conclude that bias due to sample selection is likely to be small.

[31] The full survey is provided in Appendix H.

[32] Researchers followed strict procedures to ensure the safety of victims of DV and conducted the interview only if the interviewee ensured the researcher that the perpetrator was not on the premises and after the location of the victim had been recorded. If the connection to the victim's mobile phone was interrupted, a rapid response police unit was sent to the premises to ensure the safety of the interviewee.

[33] This sample was negotiated with the Leicestershire Police Information Services Unit based on their resource constraints and an estimated 250 surveys.

3.6 Descriptive statistics and treatment/control group balance

We test the random assignment of cases by comparing mean characteristics between the treatment and control groups (Table 1). Based on the reported characteristics, treatment and control are well-balanced; most observables do not differ significantly between the two groups.[34] Some important characteristics reflecting incident severity and the state of household violence are worth highlighting. Specifically, the average number of cases over the last year (2.33 and 2.26) and the responding officer's risk assessment score (1.28 and 1.28) do not differ significantly between treatment and control. Furthermore, we do not observe a significant difference in the intimate partner status of the victim and perpetrator or the presence of children in the household. Importantly, the pooled F-stat from a regression of treatment status on all characteristics used in the main analysis fails to reject that the treatment and control group are balanced ($p = 0.662$). We therefore interpret Table 1 as evidence that allocation to the treatment or control group was random.[35]

Table 1 about here

The descriptive statistics for this sample are consistent with the picture of demographic characteristics of victims and perpetrators in previous studies. In total, 87% of victims versus 14% of perpetrators are female. On average, victims are slightly older than perpetrators (34.5 years versus 33.2 years). The victim and perpetrator are intimate partners in 77% of cases, and cohabiting at the time of the initial callout in 55% of cases. In all, 58% of the sample households with children have an average of 1.95 children each.

[34] Two exceptions should be noted. First, at the time of the initial callout, perpetrators in the treatment group have 1.16 more registered instances of DV than do perpetrators in the control group. Second, victims and perpetrators are 6 percentage points more likely to be living together in the treatment group than in the control group. At a 5% level of significance, the number of significant differences is roughly what one would expect to occur by chance. The remaining differences are both statistically insignificant and small in magnitude.

[35] In Appendix F.1, we provide additional evidence that the treatment status is randomly distributed across the 68 neighbourhoods (police beats) represented in the data.

3.7 Characteristics by engagement status

We report differences in the pretreatment characteristics of subjects in the treatment group by their engagement with the intervention (Table 2). We divide the treatment group into three categories, subjects for whom a caseworker was unable to make contact (no contact), subjects whom the caseworker contacted, but who declined the service (contact with no engagement), and subjects who were contacted by a caseworker and the service was accepted (contact with engagement).

Table 2 about here

For many of the characteristics, we do not see a significant difference across the three groups. However, there are notable exceptions. The sex of the victim and the perpetrator appear to be important. Victims are female in 91.6% of cases with contact and engagement, but only 82.7% cases without contact ($p = 0.005$ for the difference). Similarly, 22.0% of cases with no contact have a female perpetrator compared to 9.0% of cases with contact and engagement ($p = 0.000$ for the difference). We also find that cases with no contact are 10.7 percentage points more likely to have a white victim ($p = 0.002$) and 12.4 percentage points more likely to have a white perpetrator ($p = 0.002$).

Perhaps most interesting, we find that the risk assessment score is significantly lower for no contact subjects than for subjects who are contacted. There is a 14.8% difference between the mean value of the risk assessment score in the no-contact group and the contact with engagement group ($p = 0.002$). Cases in the contact with no engagement group also had a higher risk assessment score than those in the no-contact group, equating to a difference of about 8.5% ($p = 0.086$). Put another way, 84.5% of no contact cases received the lowest risk score, as opposed to 75.8% and 72.8% of contact without and contact with engagement.

In Appendix F.2 we regress an indicator for the different margins of engagement status on all characteristics to assess the joint significance. The results are similar to what is reported in Table 2. In cases with female perpetrators, the victim is less likely to be contacted (p = 0.059), but we see no difference in engagement conditional on contact (p = 0.755). Age of the victim and perpetrator is not associated with contact rates, but engagement when contacted is increasing with age. Finally, conditional on other characteristics, a higher risk assessment score is associated with a greater likelihood of making contact (p = 0.011) and a higher engagement rate when contacted (p = 0.148).

4 Results

In this section, we discuss the estimated impact of the intervention on a number of outcomes reflecting various stages of the case life cycle. Estimates are interpreted as an intention to treat (ITT), denoted by γ_1 in the linear probability regression (1).

$$S_i = \gamma_0 + \gamma_1 treat_i + X_i'\Gamma + e_i \tag{1}$$

S_i reflects the outcome measure under consideration. $treat_i$ is an indicator equal to 1 if i was assigned to the treatment group and 0 if i was assigned to the control group. X_i denotes a vector of variables including victim and perpetrator sex, victim and perpetrator age, a *white* race indicator for victim and perpetrator, an indicator for cohabitation, an indicator for children being present in the household, the number of police-reported domestic incidents in the previous year, the risk assessment score for the initial callout, and dummy variables for the location of the initial callout across 68 neighbourhoods (police beats).[36] e_i captures all other influences on the respective outcome y_i that are unobserved by the researchers. e_i and the randomly assigned $treat_i$ are assumed to be uncorrelated.

[36]Some of these variables contain a small number of missing values. In these cases we set the missing equal to 0, and include a corresponding missing dummy equal to 1 for missing values and 0 otherwise. X_i includes the full set of these dummy variables.

4.1 Intervention effect on statement provision

In Table 3, we report the estimated treatment effects for the provision of victim statements to police. The unconditional difference between treatment and control (Column 1) shows that there is a 6.2 percentage point decrease in statement provision between the treatment and control group ($p = 0.026$). The coefficient is very similar when control variables are added, and this effect indicates that the intervention led to a 6.5 percentage point decrease in the provision of statements by victims to the police ($p = 0.014$). This corresponds to a 21.7% decrease relative to statement provision in the control group. In Appendix F.4, we provide two-stage least square estimates to examine the treatment response of victims who engage in the intervention. Under reasonable assumptions, we find a 12.6 percentage point decrease ($p = 0.012$) in statement provision for victims who engaged with the intervention.

Table 3 about here

Given the timing of the intervention, we should not observe an effect on statements that are provided to the police prior to contact with the caseworker. We test this by estimating the ITT for making a statement during the initial police callout ($day = 0$, before treatment) and (conditional on *no statement* at $day = 0$) making a statement at least one day after the initial police callout ($day > 0$, after treatment). As expected, the treatment and control group provide statements at $day = 0$ at the same frequency (Column 4, Table 3). The estimated treatment effect is a statistically insignificant difference of -1.1 percentage points ($p = 0.621$). The treatment group is less likely than the control group to make a statement at $day > 0$ (Column 5, Table 3). The estimated treatment effect is -6.2 percentage points, confirming that the difference in statement making estimated earlier arises solely from any difference arising after the initial police callout as expected ($p = 0.011$).

Figure 3 about here

We examine the timing of statements further in Figure 3 by looking at the difference in statement provision between the treatment and control groups for ten days following the initial callout (*day 0*). In Figure 3(a), we plot the probability of a statement (conditional on no statement in previous days) against days since the initial incident. In Figure 3(b), we plot the treatment-control group differences in the probability of statement provided corresponding to each day. These figures draw attention to several points. First, both the treatment and the control group exhibit a similar pattern of the propensity of early statement making that dissipates rapidly over time. By $day = 4$, the propensity to make a statement on a given day is less than 1%. Second, consistent with Table 3, we do not observe a significant difference at $day = 0$, the day with most statements made. Third, a negative treatment-control statement gap persists from $day = 1$ to $day = 4$ days following the initial callout; we do not observe a distinguishable statement difference in days for which statement making is relatively infrequent ($day > 4$).

4.2 Intervention effect on non-police service use, re-victimisation risk, and well-being

Non-police services cover a number of different forms of assistance. For treatment group subjects who engaged with the intervention, we have detailed information on service use following the initial incident.[37] The most common services include refuge (9.2% uptake), registering with a general practitioner (12.3% uptake), counselling services (48.4% uptake) and personal safety planning (60.5% uptake).

As discussed in Section 2, non-police services are administered by a large number of independent agencies, making the collection of administrative data for our sample infeasible. To estimate the effect of treatment on the use of non-police services we use information

[37] This information, taken from caseworker reports for the 261 subjects who engaged with the intervention, is summarised in Table A.1 of the supplementary appendix.

from the one-month follow-up survey, in which subjects self-report service uptake (Panel A, Figure 4).

Treatment effect estimates for use of non-police services are positive and non-trivial in magnitude.[38] The treatment group is 17.9 percentage points (61.7%) more likely than the control group to state they have visited their general practitioner as a result of the initial incident ($p = 0.042$). The treatment group is also 6.5 percentage points (163%) more likely to have visited the accidents and emergency department following the incident ($p = 0.056$). Subjects in the treatment group are 12.8 percentage points (21%) more likely to state they used a non-police service other than health services and they are 2.4 percentage points more to report to be confident in accessing existing DV services, both estimates are nevertheless not significant at conventional levels. We summarise these results with an index of service uptake (following Anderson, 2008). Overall, the intervention had a strong and significant positive effect on service uptake beyond what was provided directly through the intervention ($p = 0.042$).

Figure 4 about here

The survey results suggest that the intervention had a positive effect on the perceived risk of the subject being exposed to future DV (Panel B, Figure 4). Subjects in the treatment group are 19.4 percentage points (46%) less likely to be in contact with the perpetrator of the initial incident ($p = 0.018$). Treatment group subjects report being 16.2 percentage points (44%) more willing to report a future incident ($p = 0.105$). There is a minimal and insignificant positive effect on victims to say that their personal safety has improved since the initial incident. Overall, the repeat victimisation risk index suggests a significant decrease in risk for the treatment group relative to the control group ($p = 0.034$).

[38] However, our ability to get precise estimates is limited by the survey's sample size. Given the sample of 214, for variables with a mean of 50%, we require a treatment effect of over 11 percentage points to be statistically significant at the 10% level.

We also investigate changes relative to the initial incident in a variety of well-being measures (Panel C of Figure 4). We find consistent negative short-term effects of the intervention on a number of measures of well-being. The treatment group was 23.2 percentage points (46%) less likely to report an improved stress level since the incident ($p = 0.007$). We find negative effects on subject-reported mental health and quality of sleep ($p = 0.204$; $p = 0.234$). The overall index suggests that the intervention had a negative impact on the well-being of subjects in the weeks following the intervention ($p = 0.102$).

It should be noted that a short-run decrease in well-being, particularly as measured through stress, is not inconsistent with a decrease in victimisation risk as measured here. For example, leaving an abusive partner, while likely reducing the risk of future abuse, may introduce new problems for the subject. An abusive partner, for example, may have a role in the household as a provider of income, and assist in productive household activities. Leaving such a partner is likely to introduce household finance problems, which, in the short-run, some subjects may find more stressful than living with an abusive partner. The negative effect on stress is consistent with findings in the psychological literature that report higher stress levels for victims that are in the process of leaving or have recently left a perpetrator.[39]

4.3 Intervention effect on repeat police-reported domestic violence

In the previous section, we investigated the effect the intervention had on the perceived risk of future victimisation using survey data. In this section, we expand on this using police data on repeat victimisation and ask whether the intervention had an effect on future police-reported incidents.

We start by looking at the probability that at least one police-reported repeat incident is observed over a two year period. The estimated treatment effects are positive, but small and statistically insignificant (columns 1–2, Table 4). The treatment group is 2.3 percentage

[39] Anderson and Saunders (2003) provide an overview of the literature.

points more likely than the control group to have reported a repeat domestic incident; a 3.1% increase over the control group mean (p = 0.390). Next, we examine the results for the number of reported domestic incidents (columns 2–3). The coefficients are also small, positive but not statistically significant. Over two years, the treatment group reported 0.230 more domestic incidents (p = 0.294). Results are similar when we condition on at least observing one repeat incident over the two year period reducing the coefficient on the treatment effect slightly to 0.196 (columns 5–6, Table 4). This is a 5.4% increase over the control group mean of 3.582 DV incidents.[40]

Given the large standard errors, we cannot confidently rule out that the intervention led to an increase in subsequent police reported incidents of DV. However, we can compare our estimates to those of a similar intervention studied in Davis et al (2010),[41] in which a police officer team visited the household within 24 hours to offer victims assistance. They find that treatment increased repeat police callouts by 8 percentage points (33% of control mean) and the number or repeat callouts by 0.18 (39% of control mean). At 95%, the upper bounds on the estimates we report in Table 4 are 7.6 percentage points (10%) for probability of a repeat, and 0.66 (25%) for number of repeats. As a percent relative to mean values, we can rule out magnitudes as large as the point estimates reported in Davis et al (2010).

Table 4 about here

The challenge in using future repeats to draw conclusions about the state of violence in the household is that treatment may impact reporting, as indicated in the victim survey. If this is the case, then we may expect the treatment group to report incidents that are less severe than would be reported absent the treatment.[42] We examine three measures reflecting

[40] We provide a heterogeneity analysis by risk assessment score in Table F.2 and discuss the results in the appendix section F.5.

[41] We make these comparisons cautiously, as there are several important differences in the design of Davis et al (2010) and our study. In particular, the selection of the subject pool in Davis et al (2010) is not conditional on previous reported incidents.

[42] This assumes that the likelihood of reporting is increasing in incident severity.

the severity of the reported incidents.[43] These will allow us to examine whether subsequent police reported incidents of DV differ in their severity by treatment status. It should be noted that if the treatment leads to an increase in reporting, then γ_1, from the Equation (1) regression of severity, cannot be interpreted as a treatment effect. Observed changes in incident severity may rather in this case mechanically arise through the effect of treatment on the composition of the sample observed in a repeat incident.

As a first measure, we investigate the raw DASH score corresponding to the repeat incident, where a higher score (out of 20) is associated with a more severe incident. As a second measure, we investigate whether the victim passes the threshold for MARAC, the pre-existing multiagency intervention, either by a DASH score greater than 14 and/or by having at least seven police-reported incidents over 365 days. As a third measure of severity, we consider the arrest of a perpetrator by the responding officers, where we assume that incidents where the perpetrator is arrested are more severe.

Table 5 about here

For repeat police callouts, the treatment group had a DASH score 0.576 points lower than the control group (Column 2 of Table 5). This estimate is marginally significant ($p = 0.102$), and represents a non-trivial magnitude of a 9.4% reduction compared to the control group mean. Results are similar for the proportion of repeat incidents that pass the MARAC intervention threshold (2.7 percentage points less likely for the treatment group) and arrests (5.3 percentage points less likely for the treatment group), but these estimates are not statistically significant.

Estimates across all measures of severity are negative, indicating that reported incidents in the treated group are of lower average severity, even though they are not significant at conventional levels. Given these results, we cannot rule out that the increased propensity

[43] Severity measures are observed for the first five repeats in the first year following the intervention.

to report future incidents led to less severe cases being reported to police, in line with the results on the increased willingness to report from the victim survey. This would suggest that the positive, but small and statistically insignificant, effects on repeat incidents may be due to an increased propensity to report lower severity incidents.[44]

4.4 Intervention effect on perpetrator arrest, charge, and sentencing

Finally, we consider outcomes that mediate one side of the relationship between statement provision and repeat incidents. Given the decrease in statement provision due to the intervention, one might be concerned that this also leads to a reduction in punitive actions taken against the perpetrator. We examine this possibility here for subsequent perpetrator arrest by police, charges by the Crown Prosecution Service, and sentencing by the courts. Table 6 reports the estimates.

Table 6 about here

For each outcome, we estimate a negative effect that is small in magnitude, and no estimate is statistically significant. Treatment is linked to a 1.0 percentage points reduction in arrest, a 0.6 percentage points reduction in the perpetrator being charged, and a 0.3 percentage points reduction in the sentencing of the perpetrator. These magnitudes correspond to a 3.8%, 4.5%, and 3.6% decrease relative to the respective outcomes. These results suggest that there was little effect of the reduction in statement provision on punitive outcomes against the perpetrator.

[44] However, a reduction in incidents may be concealed by a large increase in reporting of minor cases previously not reported to police.

5 Discussion and interpretation of results

Two overarching concerns are targeted in this intervention. The first and most important is the long-term safety and well-being of victims of DV. The second concern regards the most efficient use of public services available to victims of DV, namely whether the intervention led to a better allocation of service use between police and non-police services.

Effect of the intervention on victim outcomes

Taken together, the results from the one-month survey indicate that the intervention had the expected effect on victims, in particular regarding engagement with specialist DV services and personal safety (Figure 4). Victims in the treatment group are more likely to have used health and non-health services one month following the initial incident and appear to be less exposed to repeat victimisation, largely driven by reduced contact with the perpetrator. We also document that the intervention increased stress levels. This is consistent with the victims in the treatment group engaging with services and making changes in their personal circumstances, which ultimately may improve their safety but may also increase short-term stress levels.

Despite the positive effects on the safety of victims documented using the survey results, we do not find a strong effect of the intervention on the longer-term quantity of police-reported domestic incidents. One possible explanation for this is that the intervention did not actually lead to a change in household violence. This interpretation contrasts with recent work in the economics literature that emphasises the role of outside options in reducing household abuse (Aizer, 2010; Bobonis, González-Brenes, and Castro, 2013; Anderberg, Rainer, Wadsworth, and Wilson, 2016). By making support services easier to access, the victim's outside options away from the relationship are improved, and the threat of leaving the perpetrator is more salient (Farmer and Tiefenthaler, 1996).

Considering this explanation, one can speculate why improving support service access might not reduce violence. A possible explanation is that the available services do not address the specific needs of repeat victims. A few empirical studies examine the effectiveness of individual support services and repeat violence. For instance, Stover, Meadows and Kaufman (2009) review the literature that looks at specific victim-support services, concluding there is little evidence that the services lead to a fall in rates of repeat victimisation. For example, it is possible that the mix of available services simply does meet victims' needs. This suggestion contrasts with our findings that the intervention led to increased uptake of specialist DV services, as documented in our survey results.

An alternative interpretation of our results is that the intervention reduced violence within the household but also increased the victim's willingness to report a future incident. This change might lead to the number of repeat incidents appearing to be unaffected by the intervention. We explored this channel by examining measures that reflect the severity of the repeat police callouts. The consistently lower measures of severity for treatment group victims, although not statistically different from zero, are consistent with this interpretation. These effects are also in line with the results from the victim survey, where we find that treated victims are more likely to report future incidents to police. The magnitude of the estimates on the severity of subsequent repeat incidents is worthy of further consideration: for treated victims, repeat incidents are at least 2.3 percentage points (3.1%) more likely to be reported, and the likelihood of a reported incident is sufficiently serious to warrant a MARAC intervention decreases by 2.7 percentage points (12.6%). It would be reasonable to consider this as an improvement in welfare for repeat DV victims.

Our lack of power makes both of these interpretations highly speculative. However, we are confident that the intervention did not lead to a worsening of safety for victims of DV. This finding is in contrast to previous studies (e.g., Davis, Weisburd and Hamilton, 2010) and is likely attributable to the careful design of the intervention ensuring that victim contact was made without perpetrator involvement.

Productivity in the use of police services

Above, we argue that the intervention did not lead to a deterioration of the safety of victims of DV, and possibly led to an improvement. Here, we discuss the results showing a significant decrease in statement provision, but no significant change in perpetrator arrests or charges. These results might be surprising given the strong positive correlation between victim statement provision and perpetrator arrests (Figure 1). Once a statement is made, it requires investigative efforts on the part of the police to determine whether to build a case for prosecution. In this way, the correlation between statements and arrests, charges or sentencing provides a measure of productivity. With this interpretation, the results are consistent with the intervention leading to a non-random change in statement provision. Victims who forgo statement provision due to the treatment have, on average, lower statement productivity than other victims.[45]

In Appendix B.2, we introduce a formal framework defining victim types according to whether they change their statement provision upon receiving treatment. Under the assumptions that (a) the probability of a perpetrator arrest or charge is weakly increasing in statement provision, and (b) conditional on statement provision, the intervention is uncorrelated with perpetrator arrest, a decrease in statements without a change in arrest means that police services are being used more efficiently. Specifically, for victims who forgo statement provision due to treatment, either their statement is less effective in resulting in criminal justice outcomes than those who make statements due to treatment, or the probability that their statements lead to a criminal justice outcome is close to zero.

We explore this interpretation further by comparing changes in outcomes for victims who provided a statement to police. Changes in these outcomes are consistent with the

[45] We say that a statement A is more *productive* than statement B if the probability of A leading to an arrest and other actions by police is higher than B.

intervention having affected the composition of the statement providers, as we argue in Appendix B.2.

Table 7 about here

Statement retraction is a plausible channel through which the findings presented in sections 4.1 and 4.4 may arise. If a victim retracts his or her statement, it is inadmissible as evidence against the perpetrator. We find a significant decrease in the retraction of statements that are provided after the initial callout, (Statements at $day > 0$, Panel A, Table 7). This suggests that statements made after the initial callout are 10.3 percentage points less likely to be retracted in the treatment group than they are in the control group (p = 0.013). Considering retraction of these statements for the control group is 12.2%, this is an 84% reduction, leaving treatment group statement retractions at only 1.9%. Furthermore, we do not see a similar reduction for statements made at the initial callout (Statements at $day = 0$, Panel A, Table 7), for which group differences are smaller in magnitude and not statistically significant.

The correlation between a statement and a perpetrator arrest is 10.5 percentage points higher for the treatment group relative to the control group (*Any statement*, Panel B, Table 7). Consistent with previous findings, this is due to a 15.6 percentage points increase (p = 0.063) in the correlation for statements made after the initial callout (Statement at $day > 0$, Panel A, Table 7). We find no significant difference between the treatment group and control group in this correlation for statements made at the initial callout.

We also examine differences in the treatment and control in the correlation between the provision of statements and perpetrator charges and sentencing (panels C and D, Table 7). The estimated differences between treatment and control are similar in size compared to the estimates for arrests, but not statistically different from zero.

Figure 6 about here

This result indicates an increase in the correlation between statements and arrests following the intervention, which we interpret as an increase in the productivity of police services. Note that this arises purely from the composition of statement-makers in treatment and control. One way in which there may be a compositional difference is across the risk assessment score. In Figure 5, we show the distribution of statement providers across the risk assessment score for the treatment and control group. The control group has a larger proportion of statement providers in the lowest risk assessment score, treatment group statement providers are more likely to have higher risk assessment scores. Consistent with this, we show in Appendix F.5, that the reduction in statements in response to the intervention can entirely be attributed to subjects with the lowest risk assessment score.[46]

6 Intervention cost

In this section, we provide details on the cost of implementing the programme and target obtaining a sense of whether the intervention provides for good use of scarce public resources. To do this, we focus on the effect of the intervention on statement-making. We contrast the direct implementation cost of the intervention with the savings from the reduction in time spent on investigations by police officers following a statement. For the purpose of this analysis, we implicitly assume that the marginal effect of these forgone statements on DV outcomes (e.g. charges and convictions of perpetrators) is zero. This is consistent with our results, particularly around the retraction of statements, and allows us to engage in a simplified cost-benefit analysis reflecting the short-run costs and benefits of the intervention for police.[47] This provides us with a baseline on which to assess how expensive this programme

[46]In Appendix F.5, we reproduce the main results of this study allowing for a heterogeneous response according to the initial risk assessment score.

[47]We refrain from a full cost-benefit analysis of the intervention due to important limitations on the available data. In particular, we do not consider the potentially large and difficult to quantify, intangible benefits to victims. As we lack information on how the use of the many different non-police services changed, and the corresponding cost of providing these services, our analysis does not consider the total change in

is.

The implementation cost of Project 360 reflects the set-up cost and the cost of running the intervention during the RCT (November 2014 to April 2015). We exclude evaluation costs, such as the victim survey. The total cost of the intervention, including labour, management and expenses was £64,631 for the six-month period (we provide the full details of the exercise in Appendix G). For the 510 victims in the treatment groups, this works out at £126.72 per victim. This is comparable to the program cost of a perpetrator focused DV intervention of £100 per-participant (Strang et al., 2017), and dwarfed by the estimated £20,000 per victim to support high-risk victims through the pre-existing MARAC intervention (CAADA, 2010).[48]

By reducing the time spent in an investigation following a statement to police, the intervention freed police resources. To get a sense of the value of this savings, we use official data on the cost of police officers from the *National Policing Guidelines on Charging for Police Services* (NPCC, 2019). The hourly cost of a full-time officer at the rank of Police Constable (the lowest rank) in 2019 was £58.99).

While we do not have information on the marginal change in police officer time due to the intervention, we can calculate the number of hours of police investigative time per statement not made that would have led to the programme breaking even with the cost of the intervention across all victims.[49] Based on an estimated effect of -0.065 on statements provided, we find that for an investigation time of 33 hours per statement, the cost of the intervention would be covered purely based on police officer time freed from investigating,

public spending. Finally, we do not consider potentially long-lived benefits and costs to police, such as may arise from the intervention improving victims' subjective perception of the police.

[48]We exercise caution in our comparisons with these other programmes, which may have different objectives and outcomes to the intervention we analyse.

[49]There are no official estimates on the number of hours per investigation available in the UK, but official estimates are available on the number of days per type of investigation to bring a case to closure. The median length of time to assign an outcome to domestic abuse-related offences for violence with injury, for example, is 17 days (Home Office, 2019).

ignoring any other positive effect the intervention may have. This is not too far off the median 20 hours of investigation time per DV case provided to us as an estimate by Leicestershire Police. This suggests that based only on police force cost-comparison, a victim focused intervention such as we study will recover roughly two-thirds of the total cost just from saving investigation time. Based on the benchmark figure of 20 hours per further investigation, the intervention saved 663 hours of police time to be allocated elsewhere over the six month period ($-0.065 \times 510 \times 20$hours).

7 Conclusions

Our experimental evidence on improving access to DV support services presented here leads to three key results. First, improving support service access for repeat victims led to a 22% reduction in statements to police. This suggests that, on the margin, victims in our subject pool treat police and non-police services as substitutes (see Appendix B). This is generally important; when service users view two different services as imperfect substitutes, barriers to access in one service (non-police services) may have a negative externality on the other service (police services).

Second, despite this decrease in statement provision, we do not see a corresponding decrease in criminal justice outcomes against perpetrators. We argue that this is consistent with the intervention having led to a more efficient use of police services by victims. One possible channel for improved efficiency is the retraction of a statement by the victim. Statement provision leads to an increase in police effort investigating and compiling a case, but retraction makes a statement inadmissible as legal evidence. Retraction is 84% lower in the treatment group than in the control group. In this way, making non-police services easier for victims to access will alleviate some of the pressure on scarce police services.

Third, unlike previous studies, we do not find evidence of an increase in household violence following the intervention. This is significant; the importance of the efficiency conclusion is moot if it comes at the expense of victim well-being. In fact, this study offers evidence that improving access to support services may improve outcomes for victims overall as evidenced by the lower risk of victimisation from the victim survey.

The findings have general implications for the provision of public services, when individuals decide between different alternative services for which ease of access differs. Several relevant examples involve public health services. For example, the choice of seeking help for an acute health problem using general practitioner services versus emergency services and differences in ease of access based on the provision on weekdays compared to the weekend.

This study also highlights the difficulty in designing policy to address persistent DV. Despite a significant improvement in the accessibility of DV support services, we find little change in future victimisation. These results serve as reminder of the exceptional complexity of the underlying root causes of DV and the limitations of interventions in breaking the persistent cycle of repeat victimisation.

References

Aizer, A., 2010. "The gender wage gap and domestic violence," *American Economic Review*, 100, 1847–1857.

Aizer, A. and P. Dal Bo, 2009. "Love, hate and murder: Commitment devices in violent relationships," *Journal of Public Economics*, 93, 412–428.

Anderberg, D., and H. Rainer, 2013. "Economic abuse: A theory of intrahousehold sabotage," *Journal of Public Economics*, 97, 282–295.

Anderberg, D., H. Rainer, J. Wadsworth, and T. Wilson, 2016. "Unemployment and domestic violence: Theory and evidence" *Economic Journal*, 126, 1947–1979.

Anderson, D. and D. Saunders, 2003 "Leaving an abusive partner: An empirical review of predictors, the process of leaving, and psychological well-being" *Trauma, Violence, & Abuse*, 4(2), 163–191.

Anderson, M.L., 2008. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects" *Journal of the American Statistical Association*, 103(484), 1481–1495.

Barr, A., and S. Turner, 2018. "A Letter and Encouragement: Does Information Increase Postsecondary Enrollment of UI Recipients?" *American Economic Journal: Economic Policy*, 10(3), 42–68.

Bettinger, E.P., B.T. Long, P. Oreopoulos, and L. Sanbonmatsu, 2012. "The role of application assistance in information in college decisions: Results from the H&R Block FAFSA experiment," *Quarterly Journal of Economics*, 127(3), 1205–1242.

Bobonis, G.J., M. González-Brenes, and R. Castro. 2013. "Public transfers and domestic violence: The Roles of private information and spousal control," *American Economic Journal: Economic Policy*, 5(1), 179–205.

CAADA (Co-ordinated Action Against Domestic Abuse) 2010. "Saving lives, saving money: MARACs and high risk domestic abuse," available at Link to report. Accessed on 22 April 2022.

Casey, R., Berkman, M., Stover, C., Gill, K., Durso, S., and S. Marans, 2007. "Preliminary results of a police-advocate home-visit intervention project for victims of domestic violence," *Journal of Psychological Trauma*, 6(1), 39–49.

CPS (Crown Prosecution Service), 2021. *Legal Guidance, Domestic abuse*, available at Link to report. Accessed on 01 December 2021.

Davis, R.C., D., Taylor, B., 1997. "A proactive response to family violence: The results of a randomized experiment," *Criminology*, 35, 307–333.

Davis, R.C. Weisburd, D., Hamilton, E.E., 2010. "Preventing repeat incidents of family violence: A randomized field test of a second responder program in Redlands, CA," *Journal of Experimental Criminology*, 6, 397–418.

Deshpande, M., Y. Li, 2019. "Who is screened out? Application costs and the targeting of disability programmes," *American Economic Journal: Economic Policy*, 11(4), 213–248.

Farmer, A., and J. Tiefenthaler, 1996. "Domestic violence: The value of services as signals," *American Economic Review*, 86(2), 274–279.

Finkelstein, A., an M. Notowidigdo. "Take-up and targeting: Experimental evidence from SNAP," *Quarterly Journal of Economics*, 134(3), 1505–1556.

Ford, D., 1983. “Wife battery and criminal justice: a study of victim decision making,” *Family Relations*, 32, 463–475.

Fugate, M., L. Landis, K. Riordan, S. Naureckas, and B. Engel, 2005. "Barriers to domestic violence help seeking: Implications for intervention," *Violence Against Women*, 11(3), 290–310.

Gracia, E., 2004. "Unreported cases of domestic violence against women: towards an epidemiology of social silence, tolerance, and inhibition," *Journal of Epidemiology and Community Health* , 58, 536—537.

Hastings, J.S., and J.M. Weinstein, 2008. "Information, school choice, and academic achievement: Evidence from two experiments," *Quarterly Journal of Economics*, 123(4), 1373–1414.

HMIC (Her Majesty's Inspectorate of Constabulary), 2014. “Everyone's business: Improving the police response to domestic abuse”, available at Link to report.

Home Office, 2019. "The economic and social costs of domestic abuse," Home Office Research Report 107, available at Link to report.

Hovel, M., Seid, A. and S. Lyles, 2006. "Evaluation of a police and social services domestic violence program: empirical evidence needed to inform public health policies," *Violence Against Women*, 12(2), 137–59.

Jaffe, P., M. Zerwer, S. Poisson, 2002. "Access denied: The barriers of violence & poverty for abuse women and their children's search for justice and community services after separation," A report prepared for the Atkinson Foundation.

Kling, J.R., S. Mullainathan, E. Shafir, L.C. Vermeulen, M.V. Wrobel, 2012. "Comparison friction: Experimental evidence from Medicare drug plans," *Quarterly Journal of Economics*, 127, 199–235.

McGuigan, M., S. McNally, and G. Wyness, 2016. "Student awareness of costs and benefits of educational decisions: Effects of an information campaign," *Journal of Human Capital*, 10(4), 482–519.

McGuire, J., E. Evans and E. Kane, 2021. "Domestic abuse and intimate partner violence: A review of police-led and multi-agency interventions" In: Evidence-Based Policing and Community Crime Prevention. Advances in Preventing and Treating Violence and Aggression. Springer, Cham. 99—159.

National Police Chiefs' Council (NPCC), 2019. "National Policing Guidelines on Charging for Police Services", report available at Link to report.

ONS, 2018a. "Domestic abuse in England and Wales: year ending March 2018," report by Office for National Statistics, available at Link to report.

ONS, 2018b. "Crime in England and Wales: year ending March 2018," report by Office for National Statistics, available at Link to report.

ONS, 2019. "Domestic abuse prevalence and trends, England and Wales: year ending March 2019," report by Office for National Statistics, available at Link to report.

Peterson, K., Davis, R., Weisburd, D. and B. Taylor, 2022. "Effects of second responder programs on repeat incidents of family abuse: An updated systematic review and meta-analysis", *Campbell Systematic Reviews*, 18(1).

Rights of Women, 2013. *Reporting an Offence to the Police: A guide to Criminal Investigations*, pamphlet published by Rights or Women, available at Link to report.

Robinson, A., and D. Cook, 2006. "Understanding Victim Retraction in cases of domestic violence: Specialist Courts, Government Policy, and Victim-Centred Justice", *Contemporary Justice Review*, 9(2), 189–213.

Stover S., Berkman M., Desai R. and S Marans, 2010. "The efficacy of a police-advocacy intervention for victims of domestic violence: 12 month follow-up data." *Violence Against Women.* 16(4), 410–25.

Stover, C., Meadows, A., and J. Kaufman, 2009. "Interventions for intimate partner violence: Review and implications for evidence-based practice." *Professional Psychology: Research and Practice*," 40(3), 223—233.

Strang, H., L. Sherman, B. Ariel, S. Chilton, R. Braddock, T. Rowlinson, N. Cornelius, R. Jarman, C. Weinborn, 2017. "Reducing the harm of intimate partner violence: Randomized controlled trail of the Hampshire Constabulary CARA experiment." *Cambridge Journal of Evidence Based Policing*, 1, 160–173.

WHO (World Health Organization), 2021. "Violence against women prevalence estimates, 2018: global, regional and national prevalence estimates for intimate partner violence against women and global and regional prevalence estimates for non-partner sexual violence against women," available at Link to report

Table 1: Descriptive statistics

	Treatment	Control	Difference	N
A. Victim characteristics				
Female	0.888	0.857	0.031	1015
			(0.021)	
Age	33.929	34.984	-1.055	1015
			(0.768)	
White	0.844	0.835	0.008	991
			(0.023)	
Domestic cases (365 days)	2.330	2.259	0.071	1014
			(0.095)	
Registered domestic cases	11.720	10.721	0.999	1015
			(0.683)	
Risk assessment score	1.275	1.280	-0.005	955
			(0.035)	
B. Perpetrator characteristics				
Female	0.139	0.138	0.001	1004
			(0.022)	
Age	33.028	33.392	-0.364	1004
			(0.744)	
White	0.803	0.819	-0.016	925
			(0.026)	
Domestic cases (365 days)	2.226	2.248	-0.022	1004
			(0.124)	
Registered domestic cases	11.891	10.727	1.163	1004
			(0.650)	
C. Household characteristics				
Same victim and perpetrator†	0.422	0.471	-0.049	1004
			(0.031)	
Intimate partner	0.761	0.798	-0.036	983
			(0.026)	
Cohabitation	0.532	0.593	-0.060	982
			(0.032)	
Children in the household	0.586	0.570	0.016	1009
			(0.031)	
Number of children‡	1.923	1.983	-0.060	583
			(0.082)	
F-stat [p-value]			0.928	[0.662]

Notes: This table reports variable means for cases in the *treatment* and *control* groups. Column *difference* reports the difference in group means; the corresponding standard error on difference is reported in parenthesis. Column *SMD* reports the standard mean difference. F-stat corresponds to a test of the joint significance of a regression of treatment status on all control variables. †Binary variable equal to 1 if the same perpetrator is observed for the same victim, 0 otherwise. ‡Number of children conditional on having at least one child.

Table 2: Characteristics by intervention engagement

	No contact (1)	Contact with no engagement (2)	Contact with engagement (3)	p-value (1) *vs.* (2) (4)	p-value (1) *vs.* (3) (5)	p-value (2) *vs.* (3) (6)
Victim female	0.827	0.916	0.916	[0.035]	[0.005]	[0.996]
Perpetrator female	0.220	0.121	0.090	[0.037]	[0.000]	[0.359]
Victim age	35.432	31.682	34.348	[0.015]	[0.366]	[0.050]
Perpetrator age	33.780	31.533	33.477	[0.119]	[0.794]	[0.130]
Victim white	0.910	0.865	0.803	[0.235]	[0.002]	[0.163]
Perpetrator white	0.881	0.812	0.757	[0.120]	[0.002]	[0.276]
Victim domestic cases (365 days)	2.163	2.561	2.249	[0.032]	[0.495]	[0.084]
Perpetrator domestic cases (365 days)	2.099	2.495	2.180	[0.084]	[0.652]	[0.161]
Intimate partners	0.777	0.755	0.759	[0.674]	[0.674]	[0.931]
Cohabitation	0.550	0.528	0.562	[0.723]	[0.809]	[0.563]
Children in household	0.508	0.607	0.623	[0.102]	[0.016]	[0.780]
Risk assessment score	1.173	1.273	1.347	[0.086]	[0.002]	[0.289]

Notes: This table reports mean values of each characteristic for cases in the treatment group, according to the victim's engagement with the intervention. p-values, reported in columns 4–6, correspond to a test that the true mean value in each corresponding columns are statistically different.

Table 3: Treatment effect for victim providing a statement to police

	(1)	(2)	Falsification test (3) day=0	(4) day>0
Treatment	-0.062	-0.065	-0.011	-0.062
	(0.028)	(0.027)	(0.021)	(0.024)
Victim female		-0.004	-0.006	0.004
		(0.045)	(0.036)	(0.039)
Perpetrator female		-0.055	-0.036	-0.026
		(0.044)	(0.035)	(0.038)
Victim white		0.087	0.059	0.040
		(0.049)	(0.039)	(0.046)
Perpetrator white		-0.081	-0.054	-0.046
		(0.047)	(0.038)	(0.045)
Cohabitation		0.121	-0.021	0.149
		(0.028)	(0.023)	(0.025)
Children in household		0.004	0.023	-0.016
		(0.028)	(0.023)	(0.025)
Domestic cases (365 days)		-0.004	-0.002	-0.002
		(0.009)	(0.007)	(0.008)
Risk assessment score		0.277	0.180	0.175
		(0.026)	(0.021)	(0.026)
Control group mean	0.299	0.299	0.137	0.188
	(0.020)	(0.020)	(0.015)	(0.019)
N	1015	1015	1015	878
R^2	0.005	0.197	0.167	0.210
Control variables ($p\,value$)†		0.000	0.000	0.000

Notes: This table reports linear probability estimates for a binary outcome, equal to 1 if the person identified as "victim" provided police with a statement, and 0 otherwise. Columns (1) and (2) report OLS estimate, unconditional and conditioning on the reported control variables. In Column (3), the outcome is equal to 1 if a statement is provided within 24 hours of the initial police callout, and 0 otherwise. In Column (4), the sample excludes cases in which a statement is provided within 24 hours of the initial police callout. Estimates in columns (2)–(4) also include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing variables. Robust standard errors are reported in parentheses.

†P-value corresponds to a test of joint significance of the control variables reported in this table.

Table 4: Repeat police-reported D.V., two-years following initial incident

	Repeats ≥ 1		Total repeats		Total repeats, conditional on ≥ 1	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.030	0.023	0.270	0.230	0.209	0.196
	(0.027)	(0.027)	(0.213)	(0.220)	(0.247)	(0.254)
Victim female		0.085		0.304		0.064
		(0.050)		(0.358)		(0.451)
Perpetrator female		-0.054		-0.257		0.043
		(0.047)		(0.337)		(0.403)
Victim white		0.055		-0.002		-0.218
		(0.047)		(0.447)		(0.481)
Perpetrator white		0.029		0.611		0.691
		(0.046)		(0.412)		(0.443)
Cohabitation		-0.052		-0.705		-0.455
		(0.028)		(0.242)		(0.281)
Children in household		0.036		-0.160		-0.442
		(0.030)		(0.263)		(0.319)
Domestic cases (365 days)		0.014		0.257		0.236
		(0.009)		(0.079)		(0.087)
Risk assessment score		-0.028		0.005		0.109
		(0.028)		(0.213)		(0.246)
Control group mean	0.749	0.749	2.681	2.681	3.582	3.582
	(0.019)	(0.019)	(0.141)	(0.141)	(0.165)	(0.165)
N	1015	1015	1015	1015	775	775
R^2	0.001	0.114	0.002	0.098	0.001	0.128
Control variables ($p\,value$)†		0.010		0.003		0.0384

Notes: This table reports estimates for the regression repeat police-reported domestic violence outcomes on treatment status. A repeat is defined as an incident recorded by the police involving the person identified as the "victim" in the initial incident. All outcomes reflect a period of two-years from the time of the initial call-out. The outcome in columns (1) and (2) is a binary variable equal to 1 if at least one repeat is observed, and 0 otherwise. The outcome in columns (3) and (4) is the total number of repeat police callouts for domestic violence. The outcome in columns (5) and (6) is the total number of repeat police callouts for domestic violence, conditional on at least one. Estimates in columns (2), (4), and (6) include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing control variables. Robust standard errors are reported in parentheses.

†P-value corresponds to a test of joint significance of the control variables reported in this table.

Table 5: Incident severity at repeat police callouts

	DASH score		MARAC threshold		Perpetrator arrested	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.410	-0.576	-0.014	-0.027	-0.031	-0.053
	(0.358)	(0.351)	(0.035)	(0.037)	(0.039)	(0.042)
Victim female		0.188		-0.045		-0.003
		(0.552)		(0.078)		(0.074)
Perpetrator female		-1.781		-0.106		-0.086
		(0.560)		(0.064)		(0.066)
Victim white		-0.735		-0.236		-0.091
		(0.631)		(0.071)		(0.073)
Perpetrator white		0.565		0.163		0.053
		(0.561)		(0.060)		(0.071)
Cohabitation		0.456		0.007		0.028
		(0.376)		(0.039)		(0.045)
Children in household		0.948		-0.026		0.071
		(0.391)		(0.042)		(0.046)
Domestic cases (365 days)		-0.128		0.015		0.013
		(0.109)		(0.012)		(0.013)
Risk assessment score		1.897		0.108		0.078
		(0.394)		(0.043)		(0.040)
Control group mean	6.039	6.039	0.215	0.215	0.457	0.457
	(0.255)	(0.255)	(0.025)	(0.025)	(0.028)	(0.028)
N	522	522	535	535	639	639
R^2	0.003	0.291	0.000	0.176	0.001	0.143
Control variables $(p\,value)^{\dagger}$		0.000		0.001		0.0389

Notes: This table reports estimates for the regression of outcomes reflecting the severity of repeat domestic incidents. The dependant variable in columns (1) and (2) is the average number of affirmative DASH risk assessment criteria across all repeats. The dependant variable in columns (3) and (4) is a binary variable equal to 1 if the victim meets the threshold to be recommended for a multi-agency meeting (MARAC) intervention, and 0 otherwise. The dependant variable in columns (5) and (6) is a binary variable equal to 1 if the perpetrator was arrested by the police during at least one repeat callout, and 0 otherwise. Estimates in columns (2), (4), and (6) include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing control variables. Robust standard errors are reported in parentheses.
†P-value corresponds to a test of joint significance of the control variables reported in this table.

Table 6: Treatment effect for perpetrator arrest, arrest with charges and conviction

	Arrested		Charged		Sentenced	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.016	-0.010	-0.009	-0.006	-0.009	-0.003
	(0.027)	(0.027)	(0.021)	(0.022)	(0.017)	(0.018)
Victim female		0.047		-0.010		-0.038
		(0.041)		(0.035)		(0.029)
Perpetrator female		-0.098		-0.038		-0.064
		(0.039)		(0.031)		(0.022)
Victim white		0.039		0.036		0.011
		(0.051)		(0.040)		(0.033)
Perpetrator white		-0.093		-0.032		-0.012
		(0.052)		(0.039)		(0.033)
Cohabitation		0.094		0.077		0.058
		(0.028)		(0.021)		(0.017)
Children in household		-0.017		0.014		0.015
		(0.029)		(0.022)		(0.017)
Domestic cases (365 days)		-0.004		-0.004		-0.005
		(0.009)		(0.008)		(0.006)
Risk assessment score		0.241		0.153		0.087
		(0.029)		(0.027)		(0.022)
Control group mean	0.263	0.263	0.133	0.133	0.083	0.083
	(0.020)	(0.020)	(0.015)	(0.015)	(0.012)	(0.012)
N	1014	1014	1015	1015	1015	1015
R^2	0.000	0.176	0.000	0.137	0.000	0.107
Control variables ($p\,value$)†		0.000		0.000		0.000

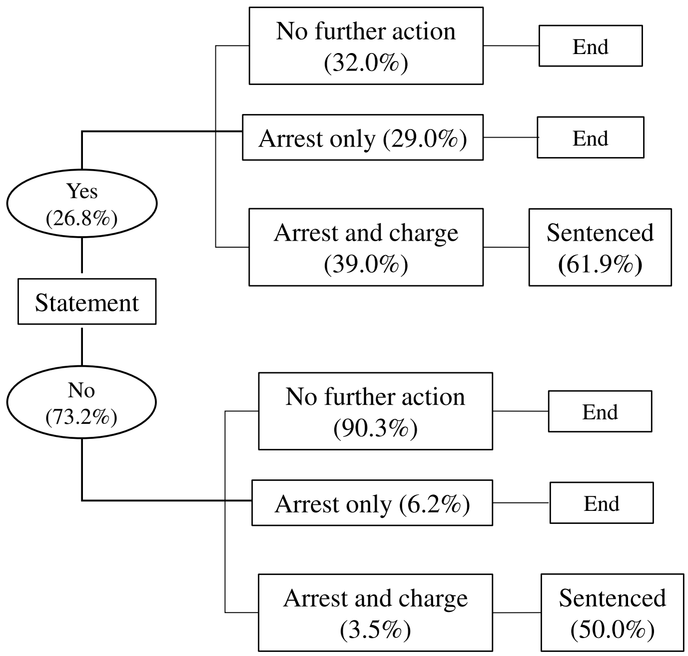
Notes: This table reports linear probability estimates for three binary outcomes, all referring to the initial callout case. Outcome *Arrest* is equal to 1 if the person identified as "perpetrator" is arrested by police, and 0 otherwise. Outcome *Charged* is equal to 1 if the person identified as "perpetrator" is charged by the Crown Prosecution Service, and 0 otherwise. Outcome *Sentenced* is equal to 1 if the person identified as "perpetrator" is convicted (fine, probation, or prison sentence) by the judiciary, and 0 otherwise. Columns report estimates of the intention to treat, unconditional and conditioning on the reported control variables. Estimates in columns (2), (4), and (6) include victim and perpetrator age, police-beat dummy variables, and binary indicators corresponding to missing control variables. Robust standard errors are reported in parentheses.
†P-value corresponds to a test of joint significance of the control variables reported in this table.

Table 7: Outcomes conditioning on statement provided by victim

	Treatment	Control	Difference
A. Statement retracted by victim			
Any statement	0.140	0.192	-0.052
			(0.045)
Statement at $day = 0$	0.235	0.275	-0.040
			(0.075)
Statement at $day > 0$	0.019	0.122	-0.103
			(0.041)
B. Perpetrator arrested by the police			
Any statement	0.744	0.636	0.108
			(0.056)
Statement at $day = 0$	0.765	0.725	0.040
			(0.075)
Statement at $day > 0$	0.717	0.561	0.156
			(0.083)
C. Perpetrator charged by the Crown Prosecution Service			
Any statement	0.397	0.371	0.026
			(0.060)
Statement at $day = 0$	0.382	0.406	-0.023
			(0.084)
Statement at $day > 0$	0.415	0.341	0.074
			(0.086)
D. Perpetrator sentenced in court			
Any statement	0.240	0.245	-0.005
			(0.052)
Statement at $day = 0$	0.221	0.290	-0.069
			(0.075)
Statement at $day > 0$	0.264	0.207	0.057
			(0.076)

Notes: This table depicts the difference between treatment and control group for perpetrator arrests, charges laid against the perpetrator and victim retraction of statements, conditioning on the provision of a statement by victim. All outcomes refer to the initial callout case. Columns labelled *treatment* and *control* report the mean for each conditional outcome for the treatment and control groups; column difference reports the difference between these two values. Rows labelled *Statements at* $day = 0$ condition on statement provided within the first 24 hours following the initial police visit, rows labelled *Statements at* $day > 0$ condition on statement provided after 24 hours period. $N = 272$, with 137 for statement at $day = 0$ and 135 for statement at $day > 0$. Robust standard errors for differences reported in parenthesis.

Figure 1: Tree representing the life of a case

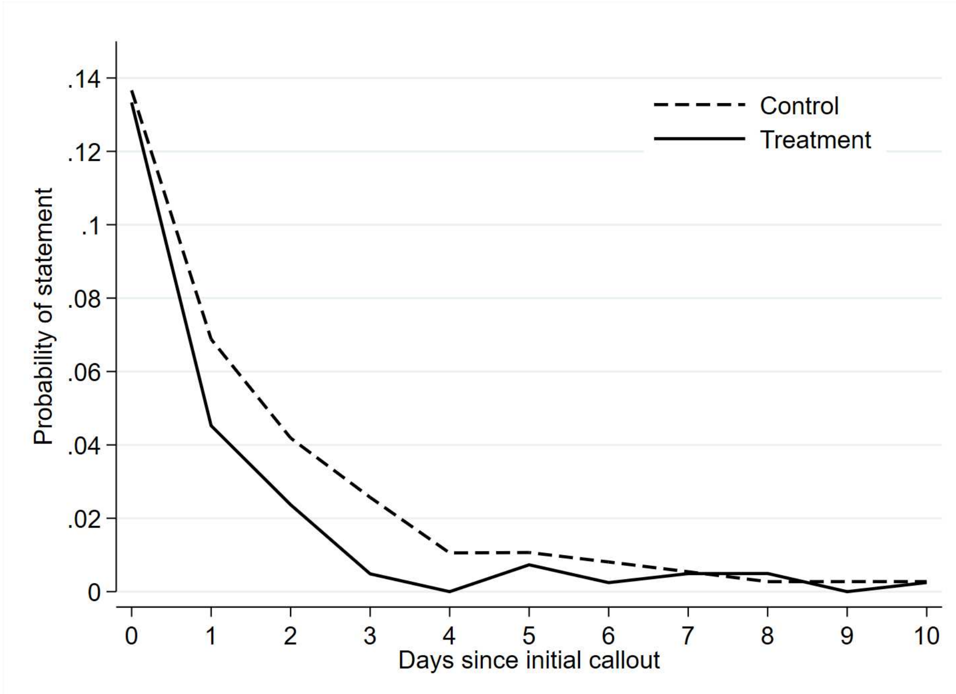


Notes: Percentages correspond to the probability of the event conditional on position in the tree, based on subject pool data. *End* nodes indicate that no further action is taken with respect to the case.

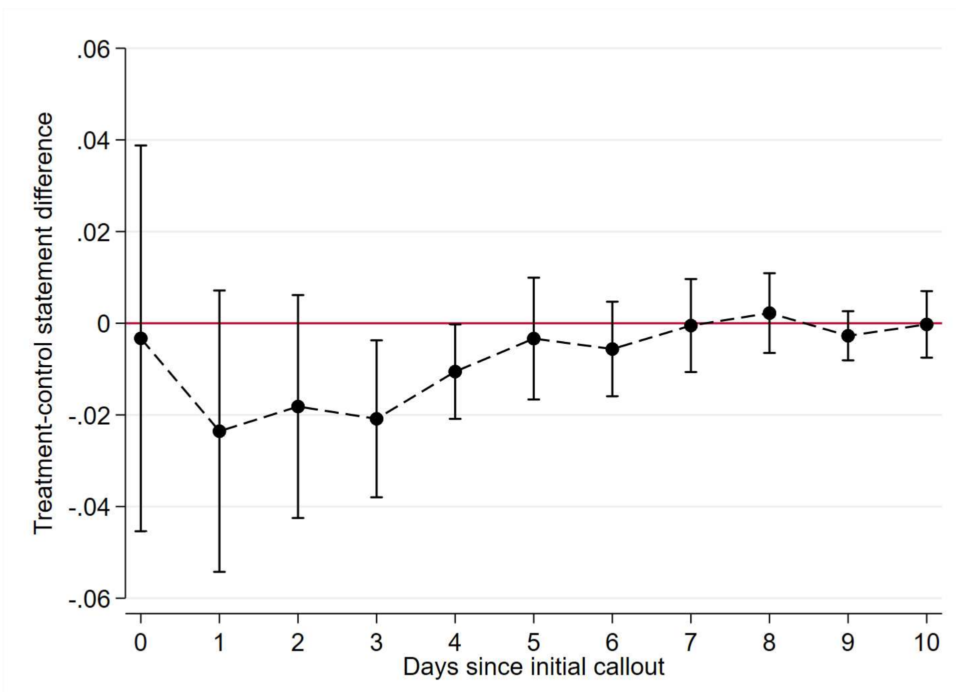
Figure 2: Leicestershire Police Force area

Notes: Map sections indicate counties for England. Area in red is the Leicester Police Force area.

Figure 3: Probability of victim statement by days since initial callout and treatment



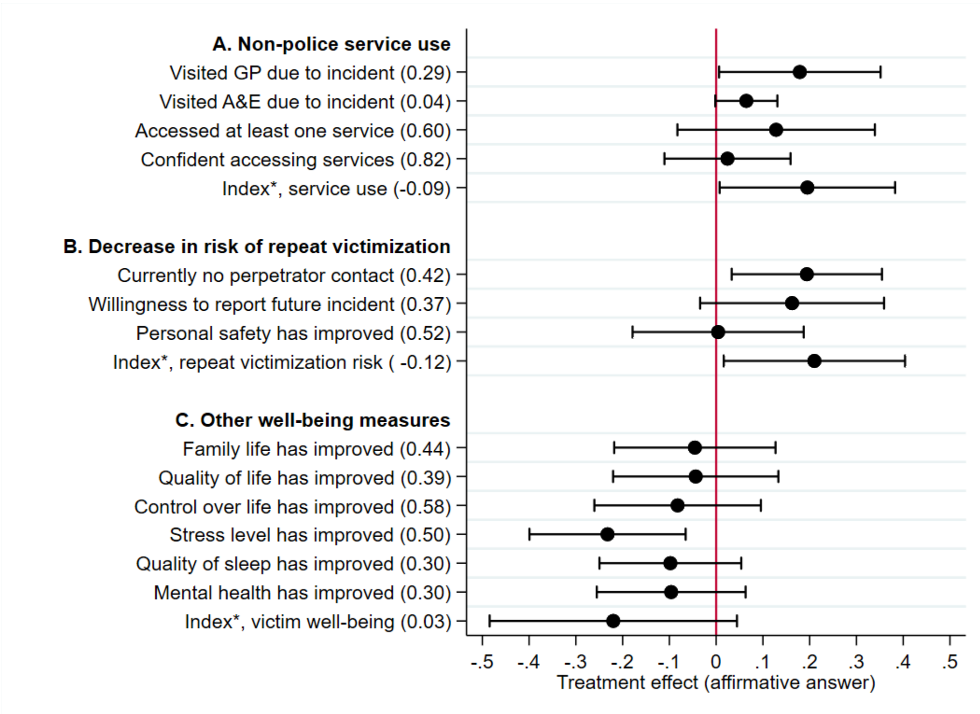
(a) Probability of statement, conditional on no previous



(b) Treatment-control group difference in the probability of statement provision, by days since initial callout

Notes: These figures show (a) the probability a statement is provided by days since the initial callout, conditional on not having not already provided a statement, and (b) the corresponding treatment-control group difference by day—bars show 95% confidence intervals on difference.

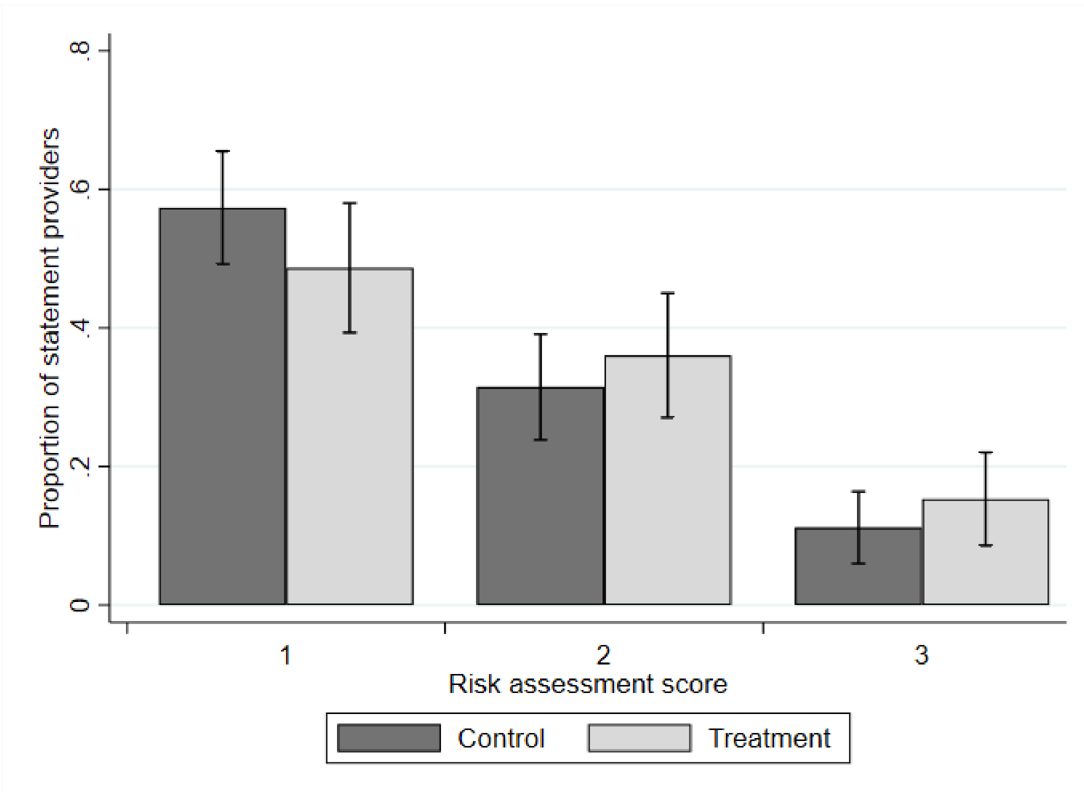
Figure 4: Non-police services and victim well-being, one-month survey



Notes: This figure reports results from selected questions on the one-month victim follow up survey. The complete survey questionnaire is available from the authors. Outcomes for each question are transformed into binary variables equal to 1 if the answer is affirmative, and 0 otherwise. Markers show the intention to treat effect (ITT); bars reflect the corresponding 95% confidence interval. Mean outcomes for the control group are reported in parenthesis. $N = 214$, with 105 in treatment and 109 in control. ITT estimates condition on characteristics X_i, described in Section 4 of the main text. *Services* are defined as any non-police services, excluding health services (GP or A&E), available specifically for domestic violence.

*Index variables are calculated following Anderson (2008).

Figure 5: Composition of statement providers by risk assessment score



Notes: In this figure bars show the percent of statement providers in each risk assessment score category, conditional on treatment status.